

which, so far as I see, are not questioned by Mr. J. W. R. The newer theory of solutions, in its entirety, is only a development of the consequences of these facts; and if errors are present therein, they can only be errors in the application, since the premisses are correct: for the proof of such errors we can, of course, in the interest of science, be only thankful.

It cannot possibly be used as an objection to the newer theory of solutions that it concerns itself at present with those properties of solutions which depend only so far upon the nature of the substance in question as one constant—the molecular weight—is concerned; and which properties, like the relation holding for gases between pressure, volume, and temperature, I have proposed to term *colligative* properties. For Mr. J. W. R. agrees with me that the number and variety of the conclusions which have been drawn from this fact of the existence of these properties is already very great, and it seems to be open to no doubt that the possible applications are by no means exhausted. It will certainly be the task of the future to take also into consideration those properties of solutions which depend upon the individual nature of substances; and this has, indeed, already to a certain extent been done in a particularly important case—that of the change with the solvent of the molecular weight of a dissolved substance, and especially that of the specific property of water to form electrolytic solutions. But I do not believe that we can be justly reproached for having endeavoured to first solve the relatively more simple problems before turning to the more complex ones. It will be here that what is termed by Mr. J. W. R. the “chemical theory” will take its proper place.

I beg Mr. J. W. R. to recall the history of the rivalry between these two “theories.” Van’t Hoff and his successors developed the laws of solutions entirely without polemical strife, because, since the fundamental ideas of van’t Hoff’s theory were entirely new, there was nothing at all in its territory to combat, as till then there was nothing there. The attacks upon van’t Hoff’s theory were begun by an investigator who had until then directed his attention exclusively to the phenomena which I have above characterized as individual, and who was evidently unprepared to deal with such colligative properties. The defence had to consist in an unceasing clearing up of misconceptions. Now, the greatest of these misconceptions is, that both “theories” are rivals. The existence and form of the laws founded by van’t Hoff and his successors stand at present beyond question; if the totality of these laws be termed the physical theory of solutions (which I should not do), there is nothing to be objected to this. But what has until now been known as the hydrate theory has not been in a position to give any information whatever in regard to these laws; none of them have been discovered with its aid, and since it has for its subject not the colligative but the individual properties of solutions this will not be otherwise in the future. In fact, the existence of the colligative laws, or van’t Hoff laws, is entirely independent of whether hydrates exist in solutions or not, and all attempts, successful or otherwise, to demonstrate the existence and composition of such hydrates, lead conversely in no wise to the van’t Hoff laws.

It will possibly not be superfluous to emphasize that with the new theory of solutions the question is not one of hypotheses, but of facts, of numerical relations. Whether one can form a conception as to the cause of the colligative laws of solutions with the aid of the kinetic molecular hypothesis or in any other way, is, for the actual existence of these laws, just as much a matter of indifference as it is for the existence of the laws of gases. In my book the question is this one of facts, and although I have therein made more use of molecular considerations than I should at present hold to be proper, yet I have done so only to render more clear the actual relations, and never to prove quantitative laws.

The *theory of solutions* which I represent and defend consists, accordingly, of a certain number of laws, *i.e.* of exact relations between measurable quantities belonging to solutions. I cannot see that that which as “chemical theory” is set in opposition thereto contains anything similar to this. The latter contains in actual material certain methods—very doubtful ones to my mind—for demonstrating the existence of compounds between dissolved substance and solvent. The result of these endeavours remains, however, in any event, entirely without influence upon those colligative laws. The same may be said of the future answer to the question, whether an interaction, and what, occurs between dissolved substance and solvent. In order to render this apparent, I need only recall the fact, re-

cently observed by Goldschmidt, that the depression of the freezing-point in a basic solvent, such as paratoluidine, caused by dissolving in it an acid, has practically the same value as that effected by the equal molecular quantity of an indifferent substance, although in the first case a chemical compound is formed and none in the second.

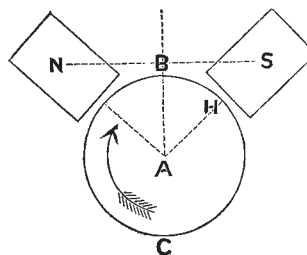
So, in the presentation of *laws* of solutions, as known up to the present, and which form the subject of my book, the so-called hydrate theory or chemical theory, did not enter into the question, because it had discovered no law of anything like general character. And further, that the methods and hypotheses of this “theory” cannot be yet looked upon as reasonably supported scientific results is known to the English-reading scientific public from a number of papers published in recent numbers of the *Philosophical Magazine*. W. OSTWALD.

Leipzig, January 4.

A Simple Heat Engine.

AT the last *soirée* of the Royal Society, a beautiful experiment was shown by Mr. Shelford Bidwell, illustrative of the fact that nickel ceases to be magnetic at a certain definite temperature. A piece of nickel was arranged as the bob of a pendulum. As long as the nickel was below a certain temperature it was held on one side by a magnet, but when it was heated by a spirit lamp-flame beyond a certain temperature it ceased to be attracted, and passed out of the range of the flame; it then cooled, and almost instantly returned to its first position, again to be released by the heat of the flame. It occurred to me that if a disk of nickel were placed in a certain position in a magnetic field, and then heated, it would continuously revolve. This on trial I found to be the case.

The experiment was arranged thus:—The nickel disk BC, 5 cm. in diameter and 1 mm. thick, was mounted on an axle passing through A; it was held in a frame so that the faces of the two poles, N, S, of an electro-magnet were at right angles to one another; the heat was applied at H; the disk revolved in the



direction shown. A great many different positions were tried, but the one indicated gave far the best results. The rotation began when the part of the disk above the flame reached 290° C. The distribution of the lines of force passing through the disk from edge to edge was examined by placing a sheet of a non-magnetic metal above the disk when cold and when heated, and sifting iron filings on to it. When cold, the lines of force were uniformly disposed across it; but when the disk was heated, only a few appeared to pass through the heated region. From this it is evident that the fall on the heated side of the disk is always less than that on the side which is not so hot, and to this the rotation is due.

FREDERICK J. SMITH.

Trinity College, Oxford, December 31, 1891.

The Migration of the Lemming.

DR. ANDREW WILSON, in referring to my letter on the Norwegian lemming, is under the very natural misapprehension—which I formerly shared—that “only a miserable remnant of the original swarm” reach the sea. Now, although it is true that throughout their pilgrimage they are exposed to the attack of every rapacious bird, beast, and biped, and that even the *Rendy*, which is by no means rapacious, never misses the opportunity of obtaining a *bonne bouche* of grass à la *saur krout* from their paunch, yet so prolific are the lemmings at this time that their numbers increase despite their enemies; some of which, be it remembered, do not dare to follow them when they leave the *field*. During the last great migration I noticed that the little pilgrims became much more numerous as they ad-

vanced towards the *fjord*; but probably no single individual of those who began the exodus lives to share its fate—only the inherited impulse survives in the offspring. These animals may live in captivity for two years; mine, at least, did so; but, so far as I know, no one else has succeeded in keeping them nearly so long; and the reasons are curious. In the first place, they fight with each other incessantly, and irrespective of sex; and secondly, they invariably defile their supply of water, so that unless this can be made *running*, they are sure to perish.

I turned out my little colony on Richmond Green, at an hour when the almost ubiquitous boy was still abed, and I watched their behaviour with a box-compass and a butterfly net. The former article proved unnecessary, as they boxed the compass for themselves, and the latter inadequate, as they ate their way through the gauze with remarkable rapidity. I should add, however, that they were all eventually recaptured, and that I derived no information as to their sense of direction from the experiment. Dr. Wilson states that naturalists generally believe that the lemmings seek a "land of promise," or rather of past fulfilment. I was under the impression that the credit (or is it the reverse?) of the idea belonged to me, but under a sun which sees so little that is new, I may well be mistaken; yet, singly or jointly, rightly or erroneously, I still believe that these migrations were formerly of benefit to the species. That they are not so now, is obvious; but the chief interest seems to lie in their periodicity, the marvellous fecundity which supports them, and the remarkable faculty which directs them.

W. DUPPA-CROTCH.

Asgard, Richmond, January 14.

P.S.—Absence from home prevented me from noticing the letter of Prof. Romanes. To the former of his two queries I reply that all the migrations which I have noticed during twenty years have *crossed* my lake, which lies nearly north and south, whereas had they followed the valley and watershed they would have been spared this labour and risk. The same argument applies to Lake Mjosen and others. As regards the second query, whether I believe in a sub-tropical Atlantis or not seems to me to have as little bearing on a possible land-connection between Norway and Iceland as on the Goodwin Sands. It has been suggested to me that at the close of the latest glacial epoch the lemmings may have found it necessary to migrate to the warmer western shores of the peninsula: this, however, leaves the presence of the animals in Iceland unexplained, save by the rather vague action of flotsam and jetsam. In any case, I only wish to adopt the most convenient hypothesis, until it is disproved or supplemented by a better one.—W. D.-C., January 18.

IN discussing the much-debated subject of the westward migration of the Norwegian lemming, the primary cause—as it appears to me—has been altogether overlooked.

This is, that the whole of Norway north of the Jotunhjem region—that is, the whole of the country of the Norwegian lemmings—is simply the steep and narrow westward slope of a long ridge of mountains.

When Mr. Collett says that "the wanderings take place *in the direction of the valleys*," he simply repeats in other words the usual description of their general westerly course.

They breed in the uplands, and when very prolific the increase must descend or perish, as they consume all the vegetation of their birth-region and no further supplies of food are obtainable either northward, southward, or eastward; but downwards, *i.e.* westward, the vegetation increases steadily as they proceed, and the descending autumn snow-line pushes onward behind them. Their devastation of meadows and oat-fields proves the urgency of their downward or westward course.

There are lemmings also on the eastern slopes of the Kjolen range, *i.e.* in Sweden. We are told that the Swedish lemmings proceed to the Gulf of Bothnia and are there drowned. To do this they must travel in the eastward and southward directions of a much longer slope than the steep westward course of the Norwegian lemming. A glance at a good map of Sweden and Norway will show all this.

W. MATTIEU WILLIAMS.

The Grange, Neasden.

The New Forest Bill, 1892.

IN connection with the petitions in favour of this Bill, to which the signatures of persons interested in the New Forest

are being obtained, I am frequently asked, "What is the necessity for the Bill, and what is its object?" The facts of the case may be shortly stated as follows. The "Woods and Wastes" of the Forest comprise about 63,000 acres of land, the whole of which were, prior to 1698, open and unclosed; but under the authority of the Acts 9 and 10 William III. c. 36 (1698) and 48 George III. c. 72 (1808), the Crown was empowered to inclose, and keep inclosed, freed and discharged from all rights of common, such quantity of land in the Forest as would amount to 6000 acres, for the growth of timber. By the Act of 14 and 15 Vict. c. 76 (the Deer Removal Act of 1851), the Crown was authorized to inclose and plant with trees any quantity of land, not exceeding 10,000 acres, in addition to the 6000 acres already in inclosure under the authority of the Acts before mentioned. The powers conferred by these Acts are not repealed by 40 and 41 Vict. c. 121 (the "New Forest Act, 1877"), but the rights of inclosure are by sec. 5 of the last cited Act limited to "such lands as are at the date of the passing of this Act inclosed, or as have, previously to such date, been inclosed by virtue of commissions issued in pursuance of the said Acts or some of them." The New Forest Act of 1877 practically secured the New Forest to the public, but the Act is virtually repealed by the 10th section of the Ranges Act, 1891 (and other Acts therein referred to), under the authority of which the War Department, with the consent of the Commissioners of Woods and Forests, can take possession of any part of the Forest for military purposes, and exclude the public from the enjoyment of any tract so taken. Already it is proposed to take 800 acres for a rifle range and the site of a camp, and there is nothing to prevent the exercise of such rights throughout the district, and the conversion of the Forest into a second Aldershot. Wherever a portion of the Forest is taken, the rights of the commoners, if they complain, will be bought up and extinguished; and thus, by taking different areas at different times, the Commissioners may before very long extinguish the common rights, and reduce the Forest into private ownership. It is clear that the proposed inclosure of 800 acres, and the user of the Forest generally in the way described, is in direct violation of the spirit and intention, as well as of the express provisions, of the New Forest Act of 1877.

The object, therefore, of the New Forest Bill is to make it clear that the Forest shall not be deemed to be within the provisions of the 10th section of the Ranges Act, 1891, and that the provisions of the New Forest Act, 1877, shall remain in force.

The rights secured by the Act of 1877 and the preservation of the Forest as an open space are of the greatest importance to naturalists, artists, and the general public, and every possible effort should be made to secure the passing of the Bill by signing petitions in support of it.

H. GOSS.

Entomological Society, 11 Chandos Street,
Cavendish Square, W., January 26.

A Brilliant Meteor.

LAST night, at 10h. 55m. G.M.T., I had the good fortune to witness the flight of a magnificently brilliant meteor. I was standing outside in the south-east re-entering corner of this building, and happened to be looking up at the constellation Leo, when the meteor suddenly flashed into sight from over the roof of the Observatory, a little east of the zenith, and not far from the stars κ and ι Ursæ Majoris, passed east of Procyon, and did not disappear till it had reached a position about 5° east of Sirius. An immediate reference to the map showed the positions of its appearance and disappearance to be about 9h. + 48° and 7h. — 15°.

For the greater part of its course it presented the appearance of a broad band of deep yellow light, but after it had passed about two-thirds of its path, it widened out into an elongated mass, distinctly rounded on the front, and of a full violet colour. From the middle of this round front the yellow band again emerged, and was finally lost to view about 15° or 20° further on. The violet mass would be about 5° in length. The whole apparition occupied 4 or 5 seconds, and the band of light was seen for an instant complete on the sky, stretching over some 65°.

About 10 minutes later a small meteor shot out from a point near the stars μ and λ Ursæ Majoris, and disappeared in the direction of Procyon.

THOMAS HEATH.

Royal Observatory, Edinburgh, January 25.